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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Two adaptive radiative transfer schemes for numerical weather prediction models" *by* V. Venema et al.

Anonymous Referee #2

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GENERAL

This manuscript presents new methods for improving the accuracy of simulated radiative fluxes at the surface in numerical weather prediction (NWP) models. Specifically, two techniques are introduced to reduce the errors resulting from the persistence assumption, in which radiative fluxes are assumed to be constant between radiative time steps: a temporal perturbation scheme and a spatial local search scheme. The latter approach seems to work somewhat better in practice.

I think this is an innovative and potentially important manuscript, well worth of publishing in ACP. However, the clarity and organization of the presentation leaves some room for improvement, mainly in sections 5 and 6, which appear to be somewhat cluttered by details.

My single major scientific comment concerns atmospheric radiative heating rates. A scheme that corrects the surface net flux but not the atmospheric heating rates seems slightly inconsistent in principle, and the short-time scale variations in heating rates could also be significant in practice. For example, if the cloud top longwave (infrared) cooling stays located where the cloud resided at the previous radiative time step, and does not move with the cloud, this would cause overestimated (underestimated) cooling in clear (cloudy) regions. If (as they authors state on p. 7254) the extension to atmospheric heating rates would be trivial for the spatial local-search scheme, heating rate results should also be discussed at least briefly. It is my intuition that the scheme might not work equally well for atmospheric heating rates as for the surface net flux (because the heating rates are less directly related to integral measures such as cloud liquid water path), but this is definitely worth testing.

Finally, a logical extension of this work would be to test the impact of the new parameterization schemes on numerical weather forecasts: take a simulation with a very short radiative time step as the reference, and investigate how fast simulations applying (i) the persistence assumption and (ii) the adaptive schemes diverge from the reference. It might be wisest to leave this for future papers, though.

SPECIFIC COMMENTS

1. Following the terminology I am used to, atmospheric radiative transfer schemes have two major tasks: computing the surface net radiative flux for the surface energy budget equation, and the atmospheric radiative heating rates for the thermodynamic equation. Therefore I find the use of the term "heating rate" disturbing in this paper, and would recommend the use of "surface net radiative flux" or "surface net flux" instead. However, if you disagree, you should stubbornly always specify that you mean the "surface heating rate", to be clear. This is more than just hair-splitting: it took me a few pages of reading before I became convinced of what you mean!

2. p. 7237, lines 21-23: Does this imply that adaptive schemes are, in general, best suited for high-resolution short-term NWP models, and less suited for coarse-resolution climate models?

3. p. 7239, first paragraph: It would be relevant to mention (either here or on p. 7241 or p. 7255) the neural networks version of the ECMWF longwave scheme (Chevallier et al. 1998, J. Appl. Meteor, 37, 1385-1397; Chevallier et al. 2000, Quart. J. Roy. Meteor. Soc., 126, 761-776).

4. p. 7243, Eq. (3): To be sure, please state explicitly that the "deltas" are absolute values of the differences. Or are they?

5. p. 7243: More generally, it is not quite clear to me how the spatial local-search scheme works. The algorithm searches for similar nearby columns, but (if I understand it correctly) it does not copy their radiative flux values directly but uses them to determine the temporal change in the flux. How does it work? E.g., even if a nearby column with "new" intrinsic calculations has similar LWP as this column right now, the change in LWP (and thus the change in the surface net radiative flux) is most likely different for the two columns. This should be clarified.

6. I found sections 5 and 6 a bit laborious to read. I think there are more details than necessary to convey the ideas. It could also help to use more informative subsection titles (e.g., "Sensitivity tests for the temporal perturbation scheme", etc.). One specific comment concerns the paragraph starting on p. 7247, line 19. As it stands now, it disturbs the flow of the text. It would better be located in a separate subsection ("Modifications of the persistence scheme", or something like that).

7. p. 7247, line 19-: This is not really a correct simulation of a "coarse-grained" radiation parameterization. I suppose that in reality, you would average the input data for the radiation calculations, not the output! Because of non-linearities, the difference could be significant. A further point (p. 7247, line 27): why an average delay of 7.5 min? It would also be worth pointing out explicitly that all model configurations you

discuss in this paragraph have the same computational costs (equally many calls to the radiation scheme), so you are really looking for the optimal combination of spatial and temporal resolution for radiation calculations.

8. p. 7249, line 25: The size of the optimal search region is 5 x 5 columns here but 7 x 7 columns on p. 7243. What is the difference?

9. p. 7250, line 14: the values of infrared RMS and bias error given here differ from those in the first line of Table 3.

10. p. 7265, Table 3. Why does the regression algorithm produce such a large bias? Is it related to the fact the coefficients are based on numerous days of data for various times of the day, but they are applied here to a single near-noon situation?

11. p. 7275, Fig. 10: One of the interesting features that should be commented is that LWP appears to be by far the most important variable for the spatial local-search scheme. Use of LWP alone would yield an RMS error of 34 W m-2 only!

TECHNICAL CORRECTIONS

1. p. 7239, lines 2 and 9: "lower ... as" should be "lower ... than". Similar errors also appear at least on p. 7248, lines 24-25 and on p. 7252, line 11.

2. p. 7241, line 1: suggestion: "computationally inexpensive".

3. p. 7244, line 9: does "subgrid-scale clouds" simply mean "cloud fraction"?

4. p. 7245, lines 17-21: better located in section 4.1?

5. p. 7247, line 9: it should be "error fields".

6. p. 7250, line 9: it should be "few intrinsic calculations".

7. p. 7250, lines 24-25: the heading used in Table 3 is "adaptive perturbation schemes", not "adaptive temporal scheme".

8. p. 7252, line 18: it should be "contributions"

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9. p. 7252, lines 23-24: it should be "... will also be given"

10. p. 7253, line 12: it should be "the same or smaller number of calls"

11. p. 7255, lines 25-26: it should be "play three roles"

12. p. 7256, lines 8-10: could be deleted. This was already stated on p. 7251.

13. p. 7257, line 20: it should be "more adaptive"

14. p. 7257, line 25: it should be "Barker et al. 2002".

15. p. 7263: In Table 1, what is "aerosols"? The aerosol visible optical depth?

16. p. 7264: In Table 2, the numerical values are misplaced by one line.

17. p. 7272, caption of Fig. 7: the bin width for the solar errors seems larger than 2 W m-2.

18. p. 7275, caption of Fig. 10: Panel (e) is total cloud cover. Also, on the following line, it should read "... varied by a factor of ten ..."

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